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ART. I.—*A History of Ileo Colitis*, as it prevailed in and around Fairfield, Wayne county, Illinois, during the summer of 1854. By S. W. THOMPSON, M.D.

GENTLEMEN—Deeming, as I do, that it is a duty imperative upon every physician to record the progress of every epidemic visitation of disease, of whatever kind it may be, or whatever character it may assume at the time of such visitation, I have thought it might not be uninteresting to you were I to give a brief sketch of the disease at the head of this article, as it prevailed during the past summer in the county where I reside. I may have been mistaken in my diagnosis of the actual morbid anatomy present; as also I may have been in the mode of practice which I thought it advisable to pursue; I may be wrong in some of the reflections and ideas which I shall here offer. But of these things you must be the judges, and should such be the case, some of you, as well as myself may learn better by seeing my errors. But to proceed.

The disease first showed itself in a serious epidemic form about the middle or last of June, 1854, and continued on with little or no abatement of its virulence until the middle or latter part of

August following. What I wish more particularly to call your attention to in this paper, is the peculiarly low and malignant character assumed by the disease in question; its little amenability to ordinary treatment, and the *early appearance* of a number of grave symptoms, usually occurring after a somewhat lengthened sickness. That there has been with us this summer a very general tendency to low or adynamic stages of disease is too well known to require further notice. What the true reasons of this disposition are, is still a mystery in part. We may if we please attempt to ascribe it to the excessively hot and dry summer appearing immediately after a cool and very wet spring. But we are still no nearer an explanation when we find that counties adjoining Wayne, and subject to the same meteorological influences, and not differing materially in the soil, remained almost entirely exempt from any serious epidemic disease for at least two months later than we did. And still more are we convinced of the insufficiency of any such explanation, when it is known that although the tendency to a low grade of disease, especially of the character of the one now under consideration, was generally manifested throughout Wayne county, yet in certain small and well-defined localities alone did there appear that peculiarly epidemic and malignant tendency of which I wish more particularly to speak.

Having prefaced by these brief remarks, I shall endeavor to describe the disease in detail as I witnessed it myself and heard it described by trustworthy and competent physicians.

The first serious case I recollect seeing was that of a man of somewhat weakly habit. He had complained of suffering from the excessive heat, and had some slight and transient diarrhœa occasionally for a fortnight, when about the first day of July he was seized with all the symptoms of dysentery. Fever, tenesmus, tormina, pulse quick and rather hard and small, not full. Stools consisted of mucus mixed with blood and a little fecal matter. Some tenderness over the region of the colon; tongue slightly coated. I did not see him at this time, but received the above account from another physician. On the morning of the fifth of July I was called in consultation. The case was then desperate, indeed hopeless. There was considerable tormina, tenesmus, and

fever; tongue was coated brown, and cracked; sordes upon the teeth; pulse small, frequent, weak and fluttering; considerable emaciation; lay in a state of stupor, partly I think the effect of opium previously given; stools were frequent and composed of greenish bloody water, sometimes almost clear blood, and at other times containing some amount of mucus. Upon pressure in the right iliac region, I discovered some tenderness, and this upon slight pressure could be traced over the region of the small bowels. The pain upon pressure in the course of the colon was not very acute. Partial perspiration would now and then appear; patechia were already plainly manifested over the chest, neck and arms. Sub-sultus tendinum was also present to a marked extent. As whatever was to be done had to be done promptly, the resisting powers of nature being rapidly upon the decline, there was administered Quinine, hydrarg. cum creta, and Dover's powder, along with turpentine. Also glysters of gum arabic emulsion and turpentine. These means entirely failed to check the discharges from the bowels, and although he appeared for a short time to revive under the stimulation of the turpentine, there shortly after appeared vomiting of the same character of matter as that passed by stool. Astringent injections, quinine, wine and turpentine, were again administered without avail, and he died on the afternoon of the seventh. From the first time I saw him, the stools were very offensive, but towards the last day they assumed the appearance of decomposed blood, and smelt so intolerably offensive as to sicken the attendants. There was present some disposition to retention of urine, but it was not serious, and I thought might be caused by a large blister over the abdomen, which had been applied and kept dressed with mercurial ointment before I saw him. This patient, towards the last few hours of life, became dull, indeed almost comatose.

The next case I saw was that of T. R., æt. 18. He was attacked on the 6th of July with tormina, tenesmus, and passage of mucus sanguinolent stools. Tongue somewhat coated with a whitish fur; some headache, but little fever; great tenderness in the track of the colon. His bowels had previously been regular. I immediately gave—

Calomel, . .	gr. xxiv.
Dover's Pulv., . .	gr. vi.
Opii, . .	gr. iv.
Plumbi acet. . .	gr. xv.

in cht. iii., one every four hours.

The patient was ordered to keep quiet, and use demulcent drinks. Under this treatment he improved rapidly for a couple of days; but at the end of that time he exerted himself too much, and was taken with a relapse. The mode of treatment pursued this time was similar to the above, but substituting more opium and ipecac., and less of the sub. mur. He again began to improve, but had some flashes of fever. I now applied a large blister over the abdomen, and substituted the hydrg. cum creta, opium and plumbi acetatis, for the medicine previously given. The blister drew well, and was followed by a decrease of all the bad symptoms. Tongue looked clean; bowels were moved only to a moderate extent, and the stools began to assume their natural color and snbsistence. Urine high-colored and copious, depositing considerable sediment upon standing. There had been some retention of urine; but this symptom was now relieved. Pulse felt soft and slow; no headache, no fever. The tormina and tenesmus had entirely disappeared. He continued to improve until the night of the 11th, when he was taken suddenly worse. There was some return of tormina and tenesmus, with pain upon pressure in the right iliac region. Fever, with a very harsh and dry skin; pulse weak quick and soft. I had ordered a dose of castor oil to be taken the evening before, but it had been neglected. I now again ordered the same, with the addition of ʒss of turpentine. He had frequent bloody, greenish, watery stools, the blood being intimately blended with the rest of the matters passed. Tongue coated brown, dry and cracked. There was present some muttering delirium, and sub sultum tendinum. I ordered, after the oil and turpentine were taken,

Hydrg. cum creta, .	gr. xv.
Morphia sulph., . .	gr. ij.
Quinine, . . .	gr. viii.

in cht. ij., one every six hours.

On the next day he was about the same. The oil brought away one dark feculent discharge, and then the passage of greenish bloody, and very offensive watery stools continued. There was some slight disposition for the tongue to clean off. Still some fever, but extremities are sometimes cold. Moans and talks in his sleep. Great restlessness. The tenderness and gurgling in the right Iliac Fossa continued and seemed to be increasing. Ordered the surface to be sponged with cold water and seidlitz powders to be taken pro re nata. Also,

Calomel, gr. vi,
Opium, gr. ij,
Quinine, gr x.
in cht iij, 4 to 6 hours.

Repeat Castor oil and Turpentine. On the 13th I found him some better. The oil and turpentine brought away some two or three dark feculent discharges, not nearly so fetid or bad colored as previously. I now ordered

Dover's powder, gr. xv,
Opium, gr. iij,
Ext. Gentian, f s,
Quinine, gr. xx.
In pills, viii, 2 every 4 hours.

Spts. Etheris Nitrici ℥j coch. parv. 2 hours. Sponging the surface with warm vinegar and water.

After this date he continued to improve, and he took no medicine except Quinine and Dover's powder.

His family afterwards removed him to a distance of twenty miles, but he did not seem to suffer materially from fatigue, and eventually slowly recovered.

I saw several other cases about this time, and all were taken in a way similar to those just described, and in all at the end of two or three days if the disease was not previously checked, there came on purging of bloody or greenish bloody water, with pain and gurgling in the right Iliac Fossa, and in many there was present general abdominal tenderness with Tympanites. The ' Tormina and Tenesmus, and passage of more or less Mucus continued after symptoms of inflammation of the small intestine came on. In

some there would be a greater predominance of symptoms referable to the colon, than in others. But it would be tedious for me to give you a history of each individual case, and I shall therefore relate but one more in detail.

On the 28th of July I was called in consultation to see a lady affected by this disease. The symptoms previous to my seeing the patient were as follows. She was taken suddenly three days before, having previously been in good health, with violent and continued pain in the course of the colon, with the passage of blood and mucus. Some considerable fever. Pulse quick and full, but not very hard. Tongue coated to a moderate extent with a yellowish white fur. Extremities at times inclined to be cold. Tenderness upon pressure, in the track of the colon. Considerable Tenesmus. At the time I saw her the symptoms were as follows. Paroxysms of pain through the bowels, more especially upon the passage of stools, which occurred every half hour or hour. High fever, but extremities occasionally cold. Some headache. Tenderness and gurgling in right Iliac fossa. Stools passed involuntarily, and were of a pinkish green offensive water, mixed with small clots of partially decomposed blood, and a considerable amount of mucus. Pulse very quick, weak and thready, yet not very soft. Marked Sub sultus Tendinum. Tongue dry, coated, brown and fissured. The patient lay as if wishing to remain undisturbed, except during a paroxysm of pain, when she cried out for some one to help her. Petechia already apparent on the breast and neck. There was also over the same part, a slight vascular eruption, the vesicles of which, soon after they had acquired the size of millet seeds, became filled with an opaque serous fluid, having somewhat the appearance of thin pus. They shortly became depressed in the centre, and they assumed every appearance of Small Pox pustules. Whenever they were ruptured, they poured out the contents and left nothing but minute red points.

The plan of treatment pursued previously to my seeing her and continued in after that time, was the administration of alterative doses of Hydrg. Sub. Mur, combined with opium, Ipecac and Quinine. Sponging the surface with cold water to relieve fever. Gum Arabic Emulsion, Turpentine and Laudanum given by the

mouth and also by glysters. Wine was allowed pro re nata. She continued to grow worse. The vesicular eruption previously spoken of continued to increase, as quickly as one crop disappeared another showing itself. The stools continued about the same, of a most offensive character and still passed involuntarily. Pulse weaker and more rapid. Retention of urine also occurred, but was relieved by an emolient poultice to the Hypogastrium. Fever about the same, some wandering and delirium. Tongue brown and cracked. Sordes on the teeth. Tenderness along colon still continued, as did the tenderness and gurgling in right Iliac fossa. Abdomen became Tympanitic, and extremities gradually cold. I applied a blister to the abdomen which drew tolerably well, but afforded no relief. The hands and feet were bathed in stimulating linaments with a temporary restoration of warmth. Quinine, Hydrg. cum Creta and Dover's Powder were continued along with wine by the mouth and injections. Aqua Camphora or Spts: Mindareris given pro re nata. Light and nourishing food ordered, as there was evidently a rapid failing of the resisting powers of nature. Vomiting of a greenish, watery and very offensive matter took place shortly after. Injections of wine and beef tea ordered to be continued; but it was all to no purpose. She continued to *exist* for several days in a state at times approaching to coma. The subsultus Tendinum &c. continued to the thirteenth day when she died.

The above are true descriptions of almost all the bad cases of the disease which presented themselves. They all began as Dysentery, but rapidly degenerated into a grade of disease, the symptoms of which in most things exactly accord with our knowledge of severe cases of Typhoid fever. But there is this marked difference between the two. In the disease we are now discussing, there was always at the commencement symptoms of severe Colitis, and these continued with more or less severity to the end. This is not the case in Typhoid fever. There is another important difference which it will be well to mention, viz. The early appearance of Petechia, Sudamina and Sub Sultus Tendinum, which usually appeared *from the third to the fifth day, and in some from the first of attack*, whereas in Typhoid fever we look for par-

allel symptoms not before the eighth day. The first and last cases related above were the only fatal ones coming under my own observation, but the fatality generally in the localities where it prevailed epidemically was truly melancholy.

As I remarked in a previous part of this paper, there are many points of resemblance between this disease and Typhoid fever. The purging of liquid stools, in many cases nearly pure blood; the disposition to low muttering delirium and general prostration, along with the occurrence of sub sultus and Patechia, would all point out the similarity. But there are still other symptoms which lead us to note this connexion still more closely. These are, first, the evidences apparent of a limited contagious nature possessed by the epidemic, and secondly, its being confined with a few exceptional cases to certain well defined localities. That the disease in question was to a limited extent contagious presents itself strongly to my mind. That it occurred independently of such cause is beyond doubt; yet the occurrence of the disease could in many instances be traced so directly to contagion, that it would be difficult to entirely throw away such evidence, even were it in direct opposition to our usually received opinions. I will mention one instance in point. The third case related in this paper, had been, so far as could be ascertained, subject to no contagious influences for some time. But three of those who were in constant attendance were attacked; two of these resided when at home in districts not subject to the disease. One of the cases proved fatal. The child of the patient in question was seized with it and died. Her mother-in-law and sister, who, besides those mentioned above as having contracted the disease were in close attendance, were also attacked but recovered. I myself contracted the same complaint, but arrested it promptly by means of large doses of opium. This, it is true is but a solitary instance, yet others occurred of almost as marked a character as the above, and there was such an evident connection between effect and cause, that I think we are hardly justified in repudiating the idea of the disease in question being to a limited extent contagious. Nor is this idea at all novel. Our best authorities upon Typhoid fever, argue in support of the disease being more or less contagious in its nature. Many of the

older writers believe dysentery to be contagious when occurring epidemically, and some of our best modern authors admit that it may be, and probably is so under certain peculiar circumstances; and, what I would ask, is so likely to give a contagious principle to dysentery as its complication with a Typhoid character of disease? Dr. Bell, although not subscribing to the doctrine of its contagious nature, says: "The most plausible argument in favor of the contagiousness of dysentery are made when the disease is associated with fevers of a Typhoid character and sometimes with Typhus itself." In the same paragraph he adds: "But in such cases dysentary, like bronchitis, is a superadded disease, a complication of the original malady, and in this view does not come within scope of investigation into the etiology of either sporadic or epidemic dysentery.

I cannot however, give an unreserved and unqualified assent to this doctrine, for it seems to me improper to make the colitis the superadded disease, when really it was the primary one. A typhoid character of fever may come on subsequently to the appearance and predominance of dysenteric symptoms, as did actually occur in the epidemic I am now recording; but certainly it would be far more proper to call the fever superadded, instead of attributing the cause to the effect, as would be done by reversing the matter as Dr. Bell intimates.

Another analagous feature to typhoid fever, possessed by the epidemic, and at the same time it is a slight evidence of its possessing a contagious character, is its confinement to certain settlements, or well defined localities or neighborhoods. This was so strictly true in Wayne Co., that except a very few scattering cases of the disease, it did not spread beyond the limits of such localities or neighborhoods as it first appeared in. For instance; in the town of Fairfield, there were few families which entirely escaped its visitation, yet here the severe cases were confined to one part of the village; the disease did not extend around the town, except in one or two instances where those so attacked had been in close attendance on others suffering from the complaint. Another neighborhood about four miles north-east of Fairfield, suffered even more severely than did the latter place; almost every case attacked here

within a space of two miles square proving fatal; nor was this fatality confined to the practice of one physician merely, for all who had much to do with the disease in this neighborhood were nearly equally unfortunate.

From closely and attentively watching the symptoms and progress of this epidemic, I am convinced that the inflammatory process, in all cases, first commenced in the *colon*, but rapidly extended itself, until it reached the *small intestines*; in some cases involving the mucous coat throughout nearly their whole extent.—In the practice of Drs. Bell and Stokes we are told that dysentery is sometimes complicated with extensive disease in the small bowels; and in the opinion of these gentlemen, what is actually denominated typhoid dysentery, *is where the colitis is either complicated typhus or (typhoid) fever or extensive lesions of the small intestines.* It is most usual to see epidemics of "typhoid dysentery" in warm seasons and climates, and I would enquire, whether the extreme heat of the past summer might not be reasonably expected, under other favorable circumstances, to give rise to eisease which would simulate those incident to tropical climates?

Upon the general treatment of the disease I shall be as brief as possible, confining myself to the practical details and abstaining from theoretical discourses and abstract ideas. It was only during the first few hours of atttack that much immediate benefit could be expected from the use of medicine. If therefore I saw at this period, I gave pretty free doses of opium combined with blue mass. The following was what I usually prepared for an adult.

℞ Pil. Hydrg. - - - ʒij
Pulv. Opii - - - gr. xii

In pills x. 2 at once and 1 every 4 hours until the discharge ceases.

In some cases where the patient had been in good health, young and robust, I preferred to use the mild chlor. of mercury in place of the blue mass. I sometimes gave ipecac or pulv. comp., but the irritability of the stomach was generally so great as to preclude the use of either. I will here mention the peculiar susceptibility that existed to the scialagogue action of mercury. In some cases after giving a dose or two I have been compelled to desist from

the use of this valuable alterant, and in others, quite a troublesome ptyalism supervened upon taking three or four doses of the remedy.

After giving the medicines mentioned above, should the bowels remain costive, I usually give a dose of castor oil and laudanum, and this in a large majority of instances was all the medication required, if the patient was seen in the commencement; but should the disease be allowed to go unchecked, it became necessary to resort to other remedies. When therefore the tongue became dry, brown and cracked; the pulse weak and irregular; when there was present fever, low muttering delirium and sub sultus, and at the same time the discharges from the bowels are bloody water, mixed with mucus, I trusted to the administration of quinine, opium and hydrg. cum creta. Spts. etheris nitrici given freely. Small quantities of quite cold water for drink, and sponging the whole surface with warm or cold water to suit the patient's fancy. Glysters I have found this season to be of little or no benefit, except in cases where there was great nervous excitability and restlessness; when a simple warm anema of gum assafœtida in water or thin starch proved of great service. Should the abdomen become tympanitic, a cloth wrung out of hot water and sprinkled with turpentine I have found to be a most admirable topical application to the bowels. Great benefit may be expected from blisters, but care is required to watch the proper moment for their application, and I am satisfied that in one or two instances I have seen harm result from their too early use. When the most severe symptoms, such as tormina, tenesmus, and discharge of bloody stools, had somewhat but not entirely ceased; when the fever was disposed to become remittent in its character, or of a lower grade than it previously had been; or when the extremities were disposed to become cold, I have found to be the most advantageous time for their application. They should be kept on until their epispastic effect is sure of being obtained, but not longer, as in the latter case the irritation excited by them may be so great as to neutralize the good effect of their derivative action.

About the same time that blisters are admissible, turpentine also effects its most valuable results. This remedy I have given

in doses of from ten drops to half a drachm every two hours, and with the most marked benefit. I usually combined it with laudanum; and the administration has been followed, in many instances, by a decreased and improved condition of the discharges from the bowels; a relaxation and softening of the skin, and clearing off of the tongue. I cannot give minute directions concerning all the little "et ceteras" required in the treatment of this or any other disease; as such things must, of course, be filled up by the good sense and judgment of the physician. I will say a word, however, upon the use of quinine in this disease. *I have used it in every case;* sometimes from the very beginning; at others not until the latter stages of the complaint. In regard to the time of its administration, I have been governed entirely by the character of the attack. If it was one in which I might reasonably expect the rapid supervention of typhoid symptoms, I commenced giving this remedy at once; otherwise I waited for the appearance of symptoms indicating its use. The administration of wine or brandy was governed by nearly the same circumstances—of course desisting from their use as long as possible, should there be high fever, with great abdominal tenderness. I did not use cups, as I did not meet with cases wherein I thought them applicable; but I knew of several cases in which they were applied, but without any benefit resulting from their use.

I have thus given a succinct, yet, I am aware, an imperfect history of the disease which so sorely afflicted the community where I reside, and the progress of which was so well calculated to make the physician doubt the boasted powers of medicine, and cause him to dive still deeper into the mysteries of disease, and try to fathom its utmost depths. Medicine seemed, in a very large proportion of cases, to have entirely lost its controling power over disease; and Death stalked, with all his nakedness, hand in hand with the dreaded pestilence. Should what I have here recorded be the means of enabling any one hereafter to treat the disease with better success than has been my melancholy duty to record, I shall be fully satisfied.

I doubt not that many opinions I have advanced in this paper will not meet with your approval; but of this I am not the mas-

ter ; and should further investigation convince me of my errors, if such I have been guilty of, I shall be most happy to acknowledge them.

ART II.—*An Address*, read before the Madison county Medical Society, November, 1854. By JOHN S. DEWEY, M.D.

GENTLEMEN,—In accordance with a regulation of this Society, requiring a contribution from each member for our mutual interest or benefit, I purpose to communicate the deductions relative to the disease called "Milk Sickness," which a number of years' practice in a locality where it has, from the first settlement of the country until the present time, been peculiarly fatal to the inhabitants, and to such of their domestic animals as are obnoxious to it, has enabled me to draw.

The greatest diversity of opinion prevails on the subject of this disease among numbers of the profession, owing to the want of the means of information. Our text-books do not admit it into the catalogue of human maladies; our medical journals seldom contain even the most meagre expression of opinion of it; and, as the cause which produces it is happily confined to circumscribed and widely separated localities, comparatively few practitioners ever have an opportunity of witnessing its phenomena. Therefore there are intelligent physicians who even doubt the existence of this disease; which has been witnessed or treated by a large number of us. According to the rules of evidence, we can establish the fact that such a disease, sui generis, does exist; by our testimony that we have seen it, *smelt it*, and anxiously watched its singular phenomena, though all the profession beside ignore it.

Its name is furnished us by the uninitiated in plain English, and is not inappropriate; for the virus is usually communicated to the human system, and brought to our notice through the medium of milk. The flesh of the infected animal seldom contains the poison sufficiently concentrated to impart its effect by the quantity usually eaten.

The name may also refer to the affinity which the poison manifests for milk. It invariably runs harmlessly through the system of the animal in which this secretion is active; but shows its viru-

lence on the offspring or individual, which takes the secretion into the stomach.

If we prefer the language usually employed in our nomenclature, we have a name sufficiently euphonic and descriptive, and which I see no good reason to exchange for any other—morbus lactealis, or morbus ab lactæ.

Various theories prevail with regard to the cause of the disease: some attribute it to a malaria which arises from the earth in certain damp and fertile localities; others, to a mineral, the sediment of which may sometimes be seen in stagnant pools, when there is a general scarcity of water; and, fortunately for this theory, cattle are driven to these places at the very season of the year when milk-sickness is most prevalent—some, to the web of an insect; and others, to a succulent plant, which grows in damp and thickly-shaded bottoms, and bears a white blossom until late in autumn, and which, if not really guilty of the evils attributed to it, is, at any rate, from its habits and association, justly and very generally suspected.

I shall not attempt to refute the various arguments used to prove, that among the first named articles, is to be found the insidious poison; but will briefly state the reasons which have convinced my mind, that it will yet be detected, and demonstrated, in the last named plant. 1st. It is invariably found in this State; in Ohio and Kentucky; whenever milksickness is endemic; and in our own county, it is demonstrable, that it flourishes most abundantly in the immediate locations where the disease is most rife. In the latter part of this summer, a friend of mine from Washington county in this State remarked, that he had not seen this plant on his journey to Jacksonville, except in a bottom about a mile south of Edwardsville; and unhesitatingly averred that *there* was Milksickness. We know the truth of his conclusion, however we may condemn his premises. He emigrated from a portion of Ohio where this disease prevails, and where this plant also flourishes. 2nd. I know that all the symptoms of the disease were produced on calves, at two several times, by feeding them with this plant for experiment. 3rd. Whenever Milksickness is endemic, the plant can invariably be found in the vicinity. And during the past unusual

season, when the parched scanty vegetation of uplands on our south, drove the cattle from their usual haunts, to the bottoms and ravines, where this suspected plant is known to grow; the disease has been brought to localities where it was before unknown.

This plant is sought with avidity by cattle late in summer, when other vegetation is matured and dry, because it is then in bloom and more succulent. The same peculiarity renders it more easily destroyed by the first hard frost of autumn, when it and the disease cotemporaneously disappear.

Much more concurrent evidence might be adduced, fortifying the same conclusion, and defacing the theories ingeniously framed to prove the mineral or malarial origin of the poison; such as the removing it from certain localities where it was known to exist, by clearing and fencing the lands for pasture. But I abstain from further speculation on this subject; believing from the interest now manifest in the subject, that we will soon be able to point with absolute certainty to the mysterious agent of so much affliction to certain portions of the west.

I intended to have learned to what class in botany this plant belongs before this meeting, and to have tried some other experiments with it, but have been prevented by procrastination.

The symptoms of Milksickness at the outset, somewhat resembles those of our autumnal fevers, with congestion of the stomach.

In both there is fever, languor, pain in the back and limbs, with a depressed and feeble pulse, but the similarity extends no further; and if we still persist in classing it among the miasmatic fevers, we are bewildered and annoyed by the inconsistency of the subsequent symptoms.

In the latter, the tongue is pointed and its edges red. In the former we are surprised, with so much gastric irritation, to find it generally relaxed, the villi long, and covered with a white coat, and the edges pallid.

In the latter, the patient complains of a sense of heat and pain in the stomach, which is relieved in some degree by the administration of an anodyne, emollientdrinks and counter irritation.

In the former, the distress is more a sense of fulness and op-

pression, from which little or no relief is afforded by the treatment above; and the practitioner is urged by the sufferer to administer an emetic to remove the imagined accumulation.

In milk sickness there is a fetor emanated, which is truly pathognominic. It pervades the apartment where the patient is confined; is readily observed by every by-stander; and, when once perceived, is not soon forgotten. It is familiar to the inhabitant of afflicted districts; and when they detect it in a sick room they unhesitatingly pronounce their diagnosis; and no argument or authority adduced by the medical attendant can induce them to change it, or in the least shake their confidence in its correctness. Indeed, their arguments in support of their conclusion would often confound the most learned and sceptical of our profession.

They tell us of our cattle (except their milch cows) affecte with trembles, and evident enervation, and having this peculiar fitor, and will often show you a calf in the place where your patient is which had partaken of milk from the same cow, with similar symptoms of enervation and the same odor; or if the calf shows no indisposition, the disease will be developed by actually exercising him about the yard. This I have more than once witnessed, and I cannot believe that conjestive fever, with the peculiar odor, trembles, and enervation of which I have spoken, could be produced for the occasion by exercise. Or they may refer you to their carniverous animals, that have eaten of the carcasses of beasts dead of this disease, manifesting the same symptoms and odor.

In autumnal fevers complicated with congestion of the stomach the breathing may be normal, or slightly hurried and short. In milksickness it is labored, deep, and difficult, and at times performed as if by a dispairing effort of the will.

The bowels are most obstinately constipated. The surface is moist, and grows more and more cool as the circulation languishes; and unless relieved by proper treatment, the patient either dies from asthenia or remains for years an invalid, subject to a recurrence of all the symptoms. from fatigue or violent exercise.

From these facts I am led to infer, that the poison acts not upon the mucous membrane of the stomach as an irritant to excite in-

flammation of the organ ; but upon the nervous system, in such a manner as partially to paralyze the power of muscular contractibility.

We see this influence in the voluntary muscles, which immediately on the attack, refuse to sustain the weight of the patient, and tremble as if outstrained in the effort. The same condition may prevail with the heart, diminishing the force of its contracttions, and with the middle coat of the bowels suspending the peristaltic action, thus producing the obstinate constipation, which is a prominent and very difficult symptom in the disease, and on which possibly depends, the sense of fulness in the stomach, and constant retching.

That the motor portion of the nervous system is susceptible to such an impression as I have here claimed for the poison producing milksickness, is proven by well established facts, showing, that certain substances do act specifically upon separate portions of the nervous system ; as digitalis in diminishing, or *upas antiar* in suspending the action of of the heart ; or, as the analogous effect of lead, in painter's cholic.

This view of the pathology will explain more of the symptoms, than any other theory which I am aware of having been advocated and is very satisfactionally corroborated by the post-mortem appearances of the viscera.

When death occurs within the first few days from the attack, I am credibly informed, there is no trace of inflammatory action, or local lesion whatever. I have never been permitted to examine the human body, but in the carcasses of animals dead of this disease, which I have examined minutely, the only abnormal appearance which I have observed with any degree of regularity was, the utter siccity of the fecal contents of the bowels. I have seen it in the ox so perfectly destitute of moisture that it would readily ignite.

The gall bladder contains healthy bile, often in large quantity ; and is not emptied into the duodenum, owing to the same condition of the muscular fibre of the cist and duct, which holds with the bowels, and prevents their contents from being expelled ; but why the absorbents should continue to take up the fluid in the ali-

mentary canal, when the function of the exhalents is suspended, I shall not attempt to explain.*

We are also referred to the nervous system for the pathology of the disease; by the recurrence of the symptoms, from exercise or fatigue, without the reapplication of the cause which originally produced them.

Of the treatment I have but little to say. With our present knowledge, we have no antidote to neutralize the poison, or substance which will counteract its depressing influence upon the nervous system; and our only alternative is, to ward off as well as may be, its effect upon certain organs when the danger is most imminent, until the instinctive healing power in the system shall come to our aid, and expel the poison by the emunctories, or persuade the restive nervous system to tolerate its stay.

The first indication then is to overcome the constipation of the bowels. By this we expect to accomplish a double purpose for good; 1st, to relieve the stomach, upon the same principle that it is relieved by reducing strangulated hernia, or removing any other obstruction to their action; and 2nd, to open an outlet for the offending matter.

In the first we are generally pleased with the result. I have often known the stomach relieved immediately on the contact of the purgative stimulants, with the extremities of the nerves of the bowels, encouraging their action.

The stomach does not usually complain while the bowels are free.

The purgative which I prefer is the proto chloride of mercury, in 20, 30, or 60 gr. doses; given in mucilage of gum arabic; because its specific gravity makes it retained, while the retching would reject almost any other substance. The only caution necessary in the use of this article is, to be sure to give enough; so that our object may be accomplished with certainty, without danger of producing ptyalism, from which I have never known any good result.

After acting freely upon the bowels, if there are periodical exa-

* Might not the absorbents act under the hydraulic law, by capillary attraction and the vital function of the exhalents be suspended?

cerbations and remissions in the symptoms, I give sulphate of quinine in the remissions; but if this periodicity does not appear as is most frequently the case, I am driven to the employment of mere placebos or innocent and as yet fruitless experiments, until the condition of the bowels require another purgation; when I repeat the pro. chlo. of mercury in doses as before.

Though the matter ejected from the stomach usually shows an acid reaction, I have never afforded any relief by the use of either, alkalies, or vegetable or mineral acids.

I use vesication over the epegastrium; but with little apparent benefit.

The sheet anchor of my hope in the treatment of this disease is purgation, and the friendly interposition of the vis medicatrix natura.

Thus have I briefly and imperfectly communicated the deductions from my observation and experience in the disease called Milksickness, which we are more frequently called to treat, and therefore have better opportunities to investigate, than our brethren in almost any other locality; and for this reason it becomes peculiarly our duty, to our profession and humanity to ferret out its cause, its true pathology and a proper plan of treatment.

Troy, Ill., Nov. 31st, 1854.

SELECTIONS.

On Tracheotomy in Epilepsia Laryngea. By MARSHALL HALL, M. D., F.R.S., &c.

I now resume the question of the institution of tracheotomy in epilepsy. In doing this, I present to the readers of *The Lancet* the results of several years of careful investigation.

I can conscientiously affirm that my hopes of relief in *this direst* of human maladies, from tracheotomy, are more sanguine than ever. I do not, and never did, expect to cure epilepsy, especially epilepsy in its inveterate stages, or with organic lesions, by tracheotomy. I did not expect much from the use of this measure amongst the "incurables" of the lunatic asylum or the workhouse. I did not expect any good results, except by accident, in the cases in which the patient was subjected to tracheotomy without a just and previous diagnosis; for it is still necessary to repeat, that it is for laryngismus, spasmodic or paralytic, and its effects, and not for epilepsy, any more than for apoplexy, that I have proposed tracheotomy as a remedy.

In a word, I proposed tracheotomy for epilepsy in its direst form, short of inveteracy and organic lesion,—in effect, whilst hope still remained. I proposed it for that form—that of the epilepsia laryngea,—assuredly expecting it to reduce it to some other milder abortive form; and my hopes have not been disappointed. In one case,—a case in a lunatic asylum, too—the fits were reduced, by "a moderate and impartial estimate, fifty per cent. in frequency, and seventy-five per cent. in severity;" and were "entirely changed, there being not the slightest lividity of the face, or frothing of the mouth," &c., &c.

In the next place, I proposed tracheotomy for the paralytic laryngismus which follows the severest attacks of epilepsy instantly endangering life; and by it life has been preserved already in many cases, beginning with that of Mr. Cane, and for the present, ending with that of Dr. Williams, of Wrexham. I say this, notwithstanding the fact that the latter patient died afterwards; for he was rescued from a first danger to succumb to a second and totally different one.

To render the proof of the efficacy of tracheotomy still more complete, it has been observed that the fits have been slighter or more severe, according as the tracheal tube was freely patent, or

obstructed by mucus—an event, from its tenacity, very apt to occur. In one case the tracheal tube fell out, the fit was severe, and the patient expired.

The effect of tracheotomy in spasmodic laryngismus is only discovered in the induced absence of fits, or in the mitigated form of the fits which do occur. In paralytic laryngismus it is different; the lividity of the countenance, the distension of the veins, the dyspnœa subside a vue d'œil: the effect is immediate, and beyond a doubt. The danger averted; life preserved.

In this case all depends on the early institution of the operation. The laryngismus arises from paralysis of the laryngeal nerves. But other branches of the pneumogastric are paralysed, and if this condition has long subsisted, the bronchial tubes become clogged with mucus, and the patient dies of bronchial, after having escaped from laryngeal, asphyxia. This was the event in Dr. Herrick's case. It occurs, too, undetected, in many cases in which the laryngismus has subsided, and for whom a vain hope is therefore entertained. I visited such a patient many years ago with my friend Dr. Webster; the fit had passed away, but a diffused bronchial rattle remained, and the patient died of bronchial asphyxia.

I have much to say hereafter on the subject of irritation and of paralysis of the pneumogastric nerve in all its branches, events still unrecognised by the profession. But I now resume the topic of this paper.

Amongst the questions in regard to epilepsy, two most important ones have never yet been considered; the first what are the precise forms of the epilepsia trachelea, and what are the abortive forms of epiplepsy when the effects of laryngismus have been superseded by tracheotomy in the epilepsia laryngea?—the second, what are the causes and modes of death in epilepsy?

I should conclude that there is usually little danger for mind, limb, or life, in the epilepsia trachelea, judging from the effects of tracheotomy in mitigating this danger in the cure of epilepsia laryngea. In several cases not only have the complexion and the general appearance and the general health been greatly improved by tracheotomy, but the impaired mind itself has become restored. This was remarkably the case in Mr. Mackarsie's, Dr. Neill's, and Dr. Bucknill's patients.

The sources of danger to life in epilepsy are—apoplectic asphyxia and, as in tetanus, exhaustion of the nervous powers—that is, coma, and laryngeal or bronchial asphyxia from paralysis of the pneumogastric nerves, and exhaustion and sinking from the violence and repetition of the attacks.

All these are questions, not for hasty and superficial criticism, but for patient and careful investigation. And now to the proper subject of this communication.

There are TWO cases of epilepsy in which the propriety and efficacy of tracheotomy admit of no *doubt*.

The *first* of these is the *epilepsia laryngea*, of inorganic origin, in its early stage, threatening mind or life, not yet involving organic change; that is, *spasmodic* laryngisums and its effects.

The same remark applies to other convulsive diseases, perpetual convulsion inclusive.

The *second* is, the epilepsia laryngea, proceeding to coma and paralytic laryngisums or stertor, augmenting the coma and endangering life.

The same remark applies to to the coma left by other convulsive diseases, and to all kinds of simple apoplexy; that is, apoplexy without lesion or ru pture of vessels, deep intoxication, narcotic poisoning, &c.

It may now be well to take a brief retrospect, and consider what has been done.

Of Mr. Cane's case, the first and most brilliant of all, life was immediately saved, and the other patient was preserved afterwards —notwithstanding some indiscretions, such as occasionally closing the trachea—from his attacks, which had previously occurred on an average five times in a fortnight.

The case of Mr. Anderson was one of the greatest inveteracy, and tracheotomy was performed altogether without diagnosis, for epilepsy not for laryngisums. Yet was its violence mitigated.

In Dr. Neill's case there was great improvement. On one occasion "he was threatened with an attack, of which he was conscious;" "removed a temporary plug," and "the symptoms disappeared!" "He made arrangements to renew his business; walked about the streets in the confidence and consciousness of a strength of mind and purpose which he had not experienced for a long period."

On another occasion a seizure occurred; the treacheal tube, it is supposed, dropped out, and the patient "died almost instantaneously."

In Dr. Herrick's case, the convulsions, after recurring every hour or two from three o'clock to nine o'clock P.M., "kept recurring every twenty or thirty minutes; the face and lips were swollen and livid; the breathing much obstructed by mucus, slow and stertorous; the veins of the face and neck much distendeded." It was concluded that "the patient could not survive an hour unless relieved." Tracheotomy was performed, and followed by "a most marked improvement," "the breathing being more natural, the countenance more livid." On the two following days there were only four convulsions, and these less protracted and severe than any previous to the operation.

Unfortunately, this patient was only relieved. He died on the third day of bronchial asphyxia.

Henceforth let us carefully select our cases. Let a just diagnosis be instituted; let there be a fair degree of hope; let the disease be laryngeal, of sufficient gravity to justify the remedy, but not inveterate, not yet involving organic changes.

I conclude by repeating that there are two cases of epilepsy in its direst forms, in which the propriety and efficacy of trcheotomy admit of no doubt: These are—first, epilepsia laryngea, with spasmodic laryngismus, threatening the extinction of mind; second, epilepsia laryngea, with paralytic laryngismus, threatening the extinction of life.

I now propose to add notes of the recent events which have illustrated the use of tracheotomy in epilepsy. These are given in the words of the observers themselves, who have pursued the inquiry.

The first document is a note of Mr. Mackarsie:—

The following simple detail will not fail to interest your readers. It affords proof of the value of tracheotomy in epilepsy laryngea, by contrasting the condition of the patient before, during and after the existence of the tracheal opening. Had this orifice been maintained freely patent, it is obvious that the loss of intellect first, and the loss of life next, would, humanly speaking have been averted!

The present note completes the account of one of the most interesting of the cases in which tracheotomy has hitherto been performed in epilepsy; and, as the able and judicious writer justly observes:—"there is much that is important to be learnt from it." There is much to be learnt from the following striking paragraphs:—

Clay-cross, September 20th, 1854.

"My Dear Sir,—For many months I have been anxious to communicate to you the results in my case of epilepsy treated by tracheotomy.

"When I last wrote to you, my patient had a recurrence of fits, but in a much mitigated form; his mind had improved, and his complexion being of a dusky hue had assumed a natural colour, only remaining perhaps a little paler than natural.

"At this period the tracheal opening was allowed to close through neglect, and the fits resumed the same form they had had before. I wished to re-open the orifice into the trachea, but was not permitted to do so. I watched my patient with intense interest, and observed him revert to his former condition; the fits became more severe, and the coma more prolonged, the dusky hue of his complexion returned and his mind again gave way, until at length I found it necesssary to remove him to an asylum.

"In this asylum my patient remained a few days, and eventually died from the frequent recurrence of the fits.

"I think there is much of importance to be learnt from this interesting case."

I shall always consider it a great honor to correspond with you.

"I remain, dear Sir, yours very faithfully,

"W. J. MACKARSIE.

"To Dr. Marshall Hall."

The second document I adduce is an extract from an able note from Dr. Bucknell, full of the deepest interest :

"Devon County Lunatic Asylum, Exminster, Sept. 24th 1854.

"MY DEAR SIR,—Both cases on which I operated for tracheotomy have died : one of phthisic, the other of epilepsy. The mode of death in the latter was very instructive. It was the second case mentioned in my paper in THE LANCET in 1853.—

"* * * * The wonderful change in the character of the fit I have described continued for a period of about nine months.—About that time her sister paid her a visit, and they had a quarrel about their mother's will. The day afterwards she was distinctly hysterical, and in the following night she had severe epileptic convulsions. These continued at intervals of ten minutes for about sixteen hours, when she died from exhaustion, (failure of the heart's action, I suppose,) distinctly not from coma. During the whole of this period, the tracheal opening was clear.—During each fit, the external muscles of respiration could be felt in a state of tonic spasm ; and it is probable that the diaphragm was also fixed. For a short period in each fit the respiration was completely arrested. I kept the patient sitting in an easy chair, to prevent the accumulation of mucus in the air-tubes.

"I quote from memory but have the case ready for publication.

"I remain, my dear Sir, yours very truly,

"J. C. BUCKNELL.

"Dr. Marshall Hall"

I have for some time been investigating the causes of death in convulsion and apoplectic diseases. The case of which a note has been given was evidently one of exhaustion of the vis nervosa. In this manner tetanus seems to destroy life. Each successive fit adds to the exhaustion induced by the previous ones, until the patient expires.

If two frogs be affected with strychnine, and one be excited, and the other left in perfect tranquility, the former dies, and the latter survives.

The different forms assumed by epilepsy require to be investigated—the tracheal, laryngeal, the abortive in cases in which tracheotomy is instituted, the syncopal, and not least, this of nervous exhaustion.

It is difficult to imagine how the respiration can be arrested without closure of the glottis. There are three modes, however, in which this may occur: one is, at a full expiration; the second, at a full inspiration; the third, the muscles of inspiration and the muscles of expiration being simultaneously and equally contracted.

In such cases the diagnosis must be established by observing the state of the larynx, of the neck, of the face, and of the cerebrum. In the absence of the laryngismus, the deep purple lividity and tumefaction, and the subsequent deep coma, &c., are equally absent, and tracheotomy of course hors de propos.

Much observation is still required to complete our knowledge of the forms of epilepsy.

The third document I beg to adduce, is an extract from an able note from Dr. Edwards.

"Grosvenor-street, Cheltenham, Sept. 26th, 1854.

"My Dear Sir,— * * * The effect of the operation was immediate, self-evident, axiomatic, to all present. Dr. Allardyce expressed his opinion then, and repeated it this morning, that not only were the effects such, but 'that without it the case must have been utterly hopeless.' * * *

"I may sum up here, that no fit of any severe character took place in my patient from the moment the tracheal opening was made, so long as the tube, of whatever kind or form, was kept perfectly clear, and so long as it was kept in the trachea; and that whenever a seizure of a severe character occurred, the tube was found invariably more or less obstructed.

"Slight abortive fits occurred; but these were so dwindled into insignificance, that the patient, having occasionally but a momentarily feeling, said he was quite well.

"The tube was now removed by the patient. The patient continued well until December, when he became comatose, sank, and died, the history of the attack being wanting.

"Believe me, dear Sir, very faithfully yours,

"Charles Edwards."

I have taken the liberty of abbreviating the last paragraph of Dr. Edwards' admirable note.

Dr. Edwards has done great good to the cause by a most important discovery, for which I feel deeply indebted to him. This is to be found in detail in *The Lancet* for 1853. It is, that the tracheal tube, and even the exterior tube when an interior one is withdrawn, is extremely apt to be clogged and obstructed by viscid mucus. A fit may then occur, and be as severe as if the operation had never been performed.

The case requires no commentary. Tracheotomy has saved—mind and life! What can be added to such a statement?

I will now beg your readers' attention to a few short paragraphs, embracing the principles which should guide us in the use of this heroic remedy in epilepsy; they are five in number:

First.—The treatment by tracheotomy should, as in ail other cases of the use of important remedies, be founded on an accurate diagnosis; the case should be unequivocally laryngeal.

Secondly.—The case should admit of remedy; it should not have have been organic in its origin, or have sustained organic change in its progress.

Thirdly.—Not only should there be no organic diseases, but the case should not be invetorate, even if it still be one of function.

For this reason I do not think the cases already consigned to an asylum or the workhouse can offer much promise of permanent benefit from tracheotomy, or from any mode of treatment. The cases must be of such date as to present hope. They must be so severe and threatening in regard to mind or life, as to present sufficient cause or reason for the operation. It will be readily understood that I do not expect the lunatic asylum or the workhouse often to furnish cases for tracheotomy. The cases in these institutions are generally already inveterate or organic.

Fourthly.—The tracheal aperture should be ample, and be sustained unequivocally FREE.

Fifthly.—Our expectations should be reasonable; we should expect to remedy laryngismus and its effects, whatever these may be, and nothing more.

If these five principles guide us, there will be an end of all discreditable controversy; the remedy will have a fair and unprejudiced trial; we shall rejoice if it prove successful, and mourn if it prove a failure.

For myself I have only to say, that I have not, in any case in which tracheotomy has been performod, had the opportunity of forming the diagnosis for myself, having never had occasion to witness a paroxysm in any of them. I believe no proper diagnosis has been established in some of them—in Mr. Anderson's for example. And what shall I say of Dr. Andrea Verga's case? Is it not lamentable to see such things brought forward on so grave an occasion?

I fear that that will long be said of tracheotomy in epilepsy which is constantly said of other operations—as that for hernia—that when instituted, it is instituted too tardily, too late. Another observation may be made; I know positively that the operation has not generally been efficient, for want of an ampler orifice, freely sustained.

Now, let us imagine a youth attacked by epilepsy—by epilepsial laryngea; let us observe that, after a certain number of these attacks, the mind begins to fail; that there is incoherence of ideas,

loss of memory, &c.; that this state of things is augmenting; that the patient is in danger of becoming a maniac or an idiot; that all other remedies have failed; that tracheotomy affords a hope. Who will hesitate to perform so simple an operation?

Or, let us imagine that, after a severe epileptic attack, the patient remains in a state of coma, his life being obviously endangered, as in the case of Mr. Cane, Dr. Herrick, Dr. Williams, &c., who will hesitate to avert this danger by performing tracheotomy? In all these three cases the danger was so averted.

I will only add, that as I understand the question better, my hopes of success become more, not less, sanguine. Only let the remedy be instituted in cases perfectly appropriate, after a just and adequate diagnosis, and in a manner perfectly efficient.

Spina Bifida, a new plan of treatment by Mr. PAGET.

From the peculiar nature of this affection, owing to the almost invariable implication of the spinal cord, or its nervous branches, as have been satisfactorily shown by Cruveilheir, Stafford, Prescott, Hewitt, and other writers, we cannot expect that any surgical operation will ever prove successful, unless in certain cases in which the nerves of the cord have no connection with the sac of the tumor, or where very few branches are in connection with it, or where the tumor may be so small that its opening of communication with the spinal canal is in accordance with the size of the tumor itself. Of 20 cases examined by Mr. Hewett in various collections, only one was free from contact with the spinal nerves, and Cruveilheir again believes, from his dissections, that the connection between the tumor and the nerves is constant. This testimony, of course, invalidates very much against operative procedure. We shall find, however, on reference to the medical journals, that cases are occasionally cured, and many modes of treatment have been adopted with or without success; I may mention, for instance, pressure by means of a hollow pad or truss, puncturing the sac, ligature around the tumor, injections of iodine, removal, or any of these combined. Dr. Brainard, of Chicago, U. S., cured 1 out of 4 cases by the iodine injections, and Velpean and Chassaignac have treated cases with great success by the same treatment, on the testimony of the Gazette des Hopitaux. Puncture and pressure in certain cases offer the best, and, in fact, the only means of cure.

The following case presents some points of interest in being operated upon in a totally different manner from any case hitherto recorded, by Mr. Paget, on the 15th July, at Bartholomew's Hospital. The patient was a stout, healthy child, 3 months old, with

a tumor the size of a large fœtal head, situated over the lower dorsal and upper lumbar vertebræ. A sub-cutaneous ligature was passed around the base of the tumor, with its two ends emerging from the superior margin of its base; these were fastened to two India rubber straps, which crossed the shoulders, and which were kept in position by a wide and long band of adhesive plaster passed around the chest. It appears that pressure upon the tumor does not in any way affect the cerebral functions of the child, and Mr. Paget concludes from this and other reasons, that the opening of communication between the cyst and spinal cord most probably is very small, and therefore favorable to the operation. His object in applying the ligature under the skin, and fasteniug the ends to the India rubber straps, is to permit of the thread cutting its way out, and thus isolating the cyst, a result likely to happen in about 14 days. Should this succeed, he will be prepared to perform another operation for the removal of the cyst. Under any circumstances, this disease is almost always fatal, and the present operation is merely an experiment which suggested itself to his mind, and he believes it may prove successful. Should it not, we are still at liberty, he says, to try something else. The child was not put under the influence of chloroform, which I candidly think was a great omission.

Since the foregoing was written, the irritation and pain from the ligature became so great as to cause the child much suffering, which ended in death four days after the operation.

Local Anaesthesia. (*Translated from the Journal de Medecine et de Chirurgie Pratiques, for the Charleston Medical Journal and Review.*)

Experiments upon local anæsthesia are still continued both in France and England, and we do not consider ourselves too sanguine, in expressing a well grounded hope that sensibiliiy may, after a while, be locally diminished and suspended, by impregnating in some way, the external and accessible organs with vapour of chloroform. We have unfortunately not witnessed ourselves any thing decisive in this matter as yet: but in England it appears that results have been more favorable. Two important communications have been addressed to the Medico-Chirurgical Society of London, and to the Surgical Society of Ireland, which show that in these places experiments upon this subject are being rapidly pushed forward, and that a large number of physicians have already obtained very remarkable results. Thus Mr. Snow has discovered that lint (charpie) impregnated with chloroform, placed upon the skin, and covered over with a bit of oiled silk, occasions at first a degree of

burning in the part; after a few moments, sensibility becomes diminished and the skin may be transfixed with pins without pain; but on pursuing his investigations, he has found that the epidermis opposes itself more or less to the anaesthetic influence, and that when it was previously removed, insensibility of the dermis was very easily determined, and it could be pierced and lacerated with impunity.

This observation is of great importance to the success of local anaesthesia; it is, moreover, in conformity with the experiments of other surgeons, and of Mr. Hardy in particular, upon *currents of anaesthetic vapors directed upon the mucous surfaces.* The effects upon these vapor *douches* have been very remarkable, and we cannot avoid making known a few of the observations communicated to the Surgical Society of Ireland by this latter physician.

We have already spoken of the instrument of Mr. Hardy, imported into France by Charriere, and by means of which the surgeon of Dublin injected anaesthetic vapors into the vagina, rectum and bladder. It is with this instrument that he has continued his experiments, and he is now in possession of a number of facts, conclusively showing the efficacy of this measure in circumstances of very varied character. Other surgeons have followed his example; thus, Dr. O'Reilly has communicated to the Society of Obstetrics many remarkable facts, and among others, the following:—A poor woman was suffering from a cancer of the rectum opening into the vagina and causing intolerable pain, against which large doses of opium had been administered. The vapor of chloroform directed upon the part, produced an instantaneous relief. It was then practicable to clean and dress the cancerous surface, a thing which its extreme sensibility had before rendered quite impossible, and this practice being continued, the patient soon found herself so much more comfortable that she was able to rise and reappear in society. An insufflation of the vapor into the rectum during fifteen or twenty minutes, sufficed to obtain abundant repose, and it was specially used for this purpose repeatedly. During three months these fumigations with chloroform have been continued, and have in no wise lost their efficacy. The patient finds herself quite improved from its use, and since its first employment there has been no necessity to have recourse to opium or any other narcotic.

Another observation, due to Dr. Ringland, is no less remarkable. A child, but two months old, was affected with a bronchite, attended with suffocation and extreme agitation, and had not slept a moment for seventy-two hours. A great number of remedies had been tried, but without the least amelioration. At length, a small quantity of the vapor of chloroform was injected into the rectum by the process of Mr. Hardy. After an interval of less than a quarter of an hour, the child became calm, and fell into a

profound sleep, which lasted for two hours. When it awoke, it took the breast, which it had constantly refused before its sleep. The employment of the anaesthetic vapors was resumed and shortly afterwards the child fell once more into a deep sleep. It was necessary to awaken it after several hours to give it the breast. This process was repeated several times and conducted to a complete cure.

A few days since, says Mr. Hardy, a physician was speaking to me of a woman who was suffering from a violent pain in the ear. She was unable to procure a moment of repose, notwithstanding the use of numerous remedies. Finally it was resolved to apply a *douche* of chloroform to the meatus. After the lapse of ten minutes the pain ceased, and the patient slept profoundly. The following day she was troubled with a return of the pain, which, however, yielded immediately to a renewed application of the vapor. Mr. Hardy has observed a case precisely similar; the patient had introduced into the meatus, some cotton steeped in chloroform, without other effect than a sensation of heat. The same substance employed in vaporous *douches*, calmed the pain entirely after about eight or ten minutes, and no further uneasiness was experienced.

In a case of tetanus, a couple of these instruments have been used, to apply the douche to both masseters at once, and in a short time it was possible to open his mouth.

But M. Hardy has not restricted himself to the application of these douches to small surfaces; he has invented an apparatus which emits these vapors, either with or without admixture of warm water, in such a manner to maintain in contact with them a large surface of the skin at a time. An aneurism of the popliteal artery first suggested to him the propriety of modifying the instrument which had so well succeeded upon a small scale and upon surfaces of a limited extent. The tumour in this case was treated by compression, and the pain resulting from the process was such that the patient was utterly unable to a low the bandaging to remain in situ. M. Hardy then bethought himself of an instrument constructed upon larger proportions than the first. By its means veritable vapor baths can be administered, nd if it be desirable to act with vigor, a whole member, the pelvis, abdomen, or chest can be entirely enveloped. M. Hardy promises himself important results from the application of these vapors of chloroform, in a host of affectio s now happily modified by means of poultices, fomentations, embrocations, &c., methods far less powerful than the anaesthetic vapous, associated or not with water or other medicates.

Many members of the society have also declared that these vapor, douches had very well succeeded with themselves. and one makes mention of the ablation of a finger, without pain to the patient.

If we may judge of the matter, by the two communications above cited, this method is received with much favor in England, and we may soon expect more important results.

Suture of Tendons. By M. CHASSAIGNAC.

Our readers may recollect a case of successful tenoraphy practiced by Professor Sedillot of Strasbourg, which was reported in a late number of this journal. In the Compte Rendu de la Societe de Chirurgie de Paris, for April, 1854, we find a second case in which this operation was successfully performed, and which appoars to us worthy of being brought to the notice of the profession.

It relates to a girl of sixteen years, who, in Nov , 1853, fell on a piece of broken glass, and received a transverse wound on the anterior aspect of the lower part of the fore arm, which, after suppurating for a time, cicatrized.

Some months subsequently the patient entered the Hospital St. Antoine with the inability to flex the index finger. It was found that the inferior end of the tendon which had been divided was attached to the cicatrix of the wound already described, and that when this cicatrix was moved, the index finger moved also. On the fourth of February, M. Chassaignac laid bare the flexor tendons, by dissecting a rectangular flap of integument from the forearm, and passed a suture through the divided portions of the flexor of the index finger. The flap was then replaced and secured by stitches, and the hand was strongly flexed. In a fortnight the wound had united, and the patient left the hospital with the perfect use of her finger. The ends of the tendon were not refreshed. This fact, when considered in connection with Dr. Mayo's case of rupture of the ligament of the patella, and our observations on tendinous ruptures in the first number of the present volume of this journal, leads to the conclusion that exact apposition is all that is necessary for the re-union of divided tendons, even when separation has existed for a comparatively long period.

BOOK NOTICES.

Bernard & Robin on the Blood, by ATLEE. Philadelphia: Lippincott, Grambo & Co., 1854: from the Publishers.

Dr. ATLEE has published his notes of M. Bernard's lectures during the winter of 1853—54, with an appendix, containing some notes of the lectures of M. Charles Robin, on the anatomy of the blood, of the blood vessels, and of the parenchymatous organs.

In looking over this little book, we find but little that is really new to American readers. The numerous letter writers at Paris during the last few years have kept the profession well informed in reference to the improvements in French science, and there seems to be but little wanting, except a systematic arrangement and classification of the facts which have been floating in our periodical literature. This, so far as the blood is concerned, we were prepared to expect in Dr. Atlee's book. Such however is not the case, perhaps necessarily from the fact that the notes were hastily taken at the time of the lectures. There is however an abundance of interesting facts, many of them of much practical value. A few of these we shall notice. Our readers are aware that one theory of the circulation is based on the hypothesis, that the blood receives an increase of temperature while passing through the lungs. This, we believe, is the fundamental idea of the *filia nata jovis* of Dr. Cartwright, although the Dr. himself repudiates the doctrine on this subject. We quote the following:

"While at the extremities the temperature varies, for certainly it is lower in the fingers in winter than in summer, in the heart it is fixed. In the right ventricle it is higher than in the left. The contrary was long supposed to be the fact, from the experiments being made on the animal after death, when the heat was leaving the body, and the right ventricle being thinner than the left, of course it cools faster. The heart being withdrawn from the body, put a thermometer in each ventricle, and place the or-

gan in water at 40° Centig, the thermometer will stand at 40° also; then remove it and leave it in a temperature of 16°, and in a short time there will be a difference of 6° in the two instruments. The difference of temperature then noted by these experiments was not owing to a difference previously existing in the blood. M. Bernard last summer experimented upon at least a dozen animals while yet alive, the instruments being passed into the brachio-cephalic artery and into the descending vena cava. The same thermometer was used, for fear of the possibility of a slight difference in instruments, and the experiment was varied in every possible way. The temperature was always higher in the right ventricle, where it was 39° or 40°. The difference (and the thermometer used was graduated to the hundredth of a degree) was never more than one-half or one third of a degree. As will be seen hereafter, the maximum of temperature is in the portal vein. This diminution of temperature is owing to the air, which cools the blood; when it does so in the fingers, why should it not do so in the lungs. M. Bernard has made his experiments in the summer when the weather was very warm, and will repeat them in order to see if there be a diminution, proportionate to the temperature of the air. To resume; the blood of mammiferous animals is warmer in the veins than in the arteries; in the portal vein, than in the aorta."

In reference to the coagulation of the blood we find the following:—

"In some circumstances the blood coagulates with great difficulty, as after certain poisons, and in animals exhausted by fatigue; and cold produces the same effect. If an animal be made to die slowly by cold, and just before death some blood be taken, it will scarcely coagulate. M. Bernard does not know what becomes of the fibrine in these cases, but notices the fact. When blood is taken from cold-blooded animals in the winter, it coagulates with great difficulty, and sometimes not at all; when taken in the summer it coagulates perfectly.

M. Bernard thinks that this coagulability is connected with the respiration, with the animal temperature; that it is proportionate to the elevation of temperature, as is shown by observations on birds, mammiferous animals, &c., and also, that in the individual, it is influenced by cold."

Is seems to us that we may account for the phenomena here observed on the hypothesis that fibrine is produced in the process of secondary assimilation and that it sustains to certain tissues the relation of excretable material, while to other structures it may

subserve the purpose of nutrition. The manner in which excretions retained in the system through functional or organic lesion of their proper organs are finally removed, is beautifully shown in the following experiment :

A singular fact noticed after the removal of the kidneys is this. In twenty-four hours about six grammes of urea and uric acid are produced in an animal, and yet if you bleed at the end of twenty-four hours after the operation, you will still find them to be in very feeble proportion in the blood. Where is it eliminated in these early times of the operation? It is in the intestines, and particularly in the gastric juice, as a fistula in the stomach of a dog enabled M. Bernard to demonstrate. Before the operation there was none in what issued from the opening, and immediately after there was. After remaining for some time in the intestine, the urea changed into ammonical salts, and what is strange, the gastric juice continued all the time to be secreted. When substances generally eliminated by the kidneys were injected into the blood, they were found in the stomach. In lower animals where there is no kidney, it is in this manner that the elimination always takes place. This operation does not much enfeeble the animal. He vomits, and has diarrhœa, and at last, when enfeebled from not eating, he becomes sick. At this time the urea is no longer eliminated by the intestine, and you find much in the blood. And thus, in individuals affected with disease of the kidneys, you often see vomitings and derangement of digestion, owing to the urea being thrown into the intestines, and there undergoing change into ammoniacal products. In diabetes you find very little urea in the urine, and M. Sigalas gave urea to such patients in the expectation of finding it there, but he did not find the quantity to be increased ; it was transformed into ammoniacal products.

M. Bernard goes on to examine the blood of the portal system, and the circumstances modifying it, the notes however contain but little if anything that is new. His discovery of the function of the liver as a manufactory of sugar is dwelt upon at some length.

M. Robin has until quite recently denied that the red corpuscles of human blood ever contain nuclei ; he has however recently discovered them in the blood of a very young human embryo. In the following letter to the author, Dr. Atlee, he makes the announcement.

"Since your departure, I have had the opportunity of obtaining four human embryos, perfectly fresh ; their lengths were 8 milli-

metres, 16 millimetres, and 25 millimetres. I made some interesting observations upon them, relative to all their tissues, and particularly to their blood. I send you, here, the *resume*, which you can translate and publish, if you think proper.

"The majority of the red globules of the blood of embryos are from 8 to 12 milliemes of a millimetre broad, and some of them are even 13 and 14 milliemes. The most of them also have one, or rarely, two nuclei. These nuclei are spherical, finely granular, from 3 to 4 milliemes of a millimetre broad; but the globules of the blood are *bi-concave* as the globules of the adult. In embryos, 25 millimetres long, already *not more than one half* of the globules have a nucleus, the others have no nucleus, and are of about the same size as the globules of the adult, that is to say 8 milliemes of a millimetre. The nucleus is rarely in the centre of the globule, but almost always a little to the side of the central concavity of each face, and they do not prevent the globule from having the flattened form of the globules of the adult. These globules fold upon themselves, or become dentated; with the greatest facility, as soon as the serum is a little changed. At the same time many among them take an ovoid form, but not at all an oval-flattened; a fact which easily distinguishes them from those of fish and of reptiles, which are oval and flattened. This form, which M. Vance told me he had seen with a professor of Vienna, does really exist, but it is an accidental deformity, taking place with facility from alterations in the serum, in the same way as the foldings and other irregularities of which I have spoken.

"The globules of the embryo, seen in a serum unchanged or but slightly modified, are elastic, and resume with rapidity their form, when, having been deformed by compression, they cease to be compressed. They often assume a very elegant form, when they are accumulated and reciprocally compressed.

"The nuclei of the red globules are unaffected by acetic acid and water, but the mass of the globule is affected as that of the globule of the adult.

"I was able to verify with ease, on these embryos, that the red globules are in no way derived from the white globules, which act very differently, have another structure, &c.

"In embryos from 40 to 50 centimetres (millimetres ?) long, the globules have already the volume, which they will always have, and no longer have nuclei. The ovoid form of which I have spoken above is generally less regular than those I have represented."

In a note on tubercle a brief but clear description is given of the microsposic character of the tubercular corpuscle.

"These corpuscles are bodies slightly irregular on the surface,

and containing no nucleus. They are polyhedric, either of equal diameters, or elongated; when elongated, they are not flattened. Their borders are dentated, and in the centre are many granulations, contained in an amorphous mass. Their diameter is from 0.007 to 0.009 millimetres, very rarely 0.010; that is to say, though such almost always exist in a specimen, they are few in number. They are very rarely rounded; when nearly so you find them slightly dentated. Their contour is well marked, quite dark. Water has no action upon them; acetic acid pales but does not dissolve them: this it is important to note, for it is thus easy to distinguish them from nuclei of any kind, for all nuclei except those of cancer are rendered darker by it. The corpuscle contains no nucleus itself, and thus can easily be distinguished from concrete pus-globules, in which acetic shows at once the nuclei.

"In addition to this, the corpuscle of tubercle is much less paled.

"Their structure is simple, an amorphous matter sprinkled with granulations. In some are found fatty granulations, that could be taken for nuclei."

In conclusion we must say that we are disappointed in the work. Perhaps we should be equally disappointed should we listen to the lectures of M. Bernard himself. It is to what an author writes, not to what he says that we must look for a correct exponent of his views.

Men are comparatively careless both of method and fact, when there is no record of their language, for this reason if for no other, we would be glad to see our public teachers write their lectures. The might possibly be less imaginative and adorned with fewer rhetorical beauties, but we think they would be more accurate and more systematic in their arrangement; two qualities of essential importance to the student.

A Treatise on Diaphragmatic Hernia:—Being an account of a case observed at the Massachusetts General Hospital followed by a numerical analysis of all the cases of this affection found recorded in the writings of medical authors, between the years 1610 and 1846. By Henry G. Bowditch, M.D., one of the physicians to the Massachusetts General Hospital; member of the societies for medical observation of Paris and Boston. Buffalo, printed by Jewett, Thos & Co, 1853.

This is the title to a monograph of 77 pages, which first appeared in the Buffalo Medical and Surgical Journal. It is printed in

good style and contains a detailed analysis of all that is at present known in reference to the rare and interesting form of hernia, of which it treats. The whole number of cases of diaphragmatic hernia which the author has been able to collect from the records of the last 250 years, is 88, one of which was observed by himself in the Massachusetts General Hospital.

This fact shows clearly enough, that hernial protusions through the diaphragm are of very rare occurrence. Of 68 cases, 26 were congenital, and 42 from accidental causes or injuries.

For obvious anatomical reasons, the hernia occurs in the *left* side of the diaphragm much more frequently than in the right.—Thus of 62 cases, 41 occurred on the left side, 18 in the right and three in both sides. Of 70 cases in which the sex is given, 53 were males and 17 females. Of the 26 congenital cases, 11 died within two hours after birth, 6 within two years, 1 at seven years, and 8 not until the adult age. Of 31 accidental cases death took place instantaneously in 4; within 24 hours in 12; within one week in 7; within one year in 5; two years and upwards 3 cases. One of the latter lived 38 years. The essay does our industrious friend, Dr. Bowditch, much credit; and we are sure it will be read with interest by all who seek an acquaintance with the rare things of our profession. D.

MEDICAL NEWS.

Æsculapian Medical Society.

The annual meeting of the Æsculapian Medical Society was held in the Baptist Church in Paris, Ills.. Oct., 18th, 1854: the President, Dr. CHARLES JOHNSON, in the chair. One of the censors being absent, Dr. S. York was nominated and elected to fill his place.

The committee appointed to revise the Constitution not being prepared to report, were continued until next meeting.

On motion the following gentleman were respectively proposed as permanent members of the Society: Drs. D. W. Stormont, James M. Steele, Charles A. Hunt, B. B. Everett, T. M. Smith, J. S. Richmond, each of whom, after being examined and recommended by the censors, was by a unanimous vote received.

On motion of Dr. Logan, the Society proceeded to the election of officers for the following year, when the following gentlemen were elected: Dr. Thos. D. Washburn, President; Dr. S. York and W. B. Duffield, V. Presidents; Dr. F. R. Payne, Secr'y; Dr. C. L. Duncan, Treas'r; Drs. Chas. Johnson, S. York and F. R. Payne, censors.

On motion, the Society adjourned to meet at 7½ o'clock P. M.

In the evening the Society met and the newly elected President was conducted to the chair. According to previous notice a respectable number of ladies and gentlemen had assembled to hear a public addess. The President introduced to the audience, Dr. S. York, who spoke in a very able and interesting manner upon the causes and nature of Quackery. After the address was delivered the audience retired.

On motion, the Secretary read a case of Strangulated Hernia, reported by Dr. S. Thompson, which was treated successfully with the knife. Also, an interesting letter addressed to the members of the Society, written by Dr. S. Thompson.

The Society then adjourned to meet at 8½ o'clock to-morrow morning.

MORNING SESSION.

On motion, the Society ordered the Treasurer to pay $16 out of the funds of the Society for the publication of Dr. F. R. Payne's address.

The reading of essays and report of cases being in order, Dr. C. A. Hunt read an able and interesting paper on "the consideration of fever in a new nosological position."

Dr. S. W. Thompson read a valuable paper on "the history of epidemic Ileo Colitis as it prevailed in Fairfield, Wayne co., Ills.," which elicited much discussion.

Dr. T. D. Washburn reported a case of Puerperal Fever."

Dr. Hamilton reported a case of complicated "Abdominal abscess."

On motion of Dr. Logan, the following resolution was unanimously adopted.

Resolved—That the hireling system is highly derogatory to the character of the Profession, to individual physicians, and detrimental to the best interests of society at large, and that it is incumbent upon all the members of this Society to use all honorable exertions to discountenance and discourage the longer continuance of said practice in our noble profession; further resolved, that it will be expected and required of each member of this society, who may heretofore have engaged in this practice, to discountenance it as soon as present engagements are fulfilled.

On motion of Dr. Logan it was unanimously

Resolved—That in the death of Jacob Lusher, this society has lost an able and efficient, honorable and worthy member; the Profession in the Wabash Valley an experienced and successful practitioner; the community a worthy and valuable citizen.

Resolved—That we deeply sympathize with the relations of our deceased brother.

Resolved—That a copy of these resolutions be furnished to the relations of the deceased by the Secretary.

On motion, resolved that Drs. Chas. Johnson, S. York and C. A. Hunt, be appointed a committe to prepare a memorial and pe-

tition to be presented to the Legislature for the suppression of Quackery, and that ey thbe requested to confer with other medical societies in this State on the subject ; and further that Dr. Johnson be requested to carry up the petition and use his influence with the members of the Legislature to ecure the passage of a law that will protect the public from the injury inflicted by medical pretenders.

On motion of Dr. Hamilton, resolved that the papers of this Society have accumulated in such quantity, and assumed such importance as to require a special committee to take supervision of them, said committee to be styled the 'publication committee,' and that all papers submitted to this society hereafter, shall be placed in the hands of that committee; and further, said committee be appointed by the chair. Whereupon the chair appointed Drs. C. L. Duncan, H. R. Payne and L. C. Churchill.

On motion, the Society adjourned to meet at 1½ o'clock, P.M.

EVENING SESSION.

On motion, Drs. C. Johnson, S. York and W. B. Duffield were appointed delegates to the National Medical Convention, to be held in Philadelphia next Spring.

On motion the following delegates were appointed to attend the State Medical Society—Drs. J. M. Logan, A. W. McClure and H. R Payne.

The following gentlemen were appointed to read essays at the next meeting of this Society—Drs. A. W. McClure, H. W. Davis, H. R Payne, C. L. Churchill, J. S. Richmond, J. D. Mitchel and D. W. Stormont.

On motion, Dr. J. S. Richmond was appointed to deliver a public address at the next meeting of the Society.

On motion of Dr. Duncan,

Resolved—That the thanks of this Society are hereby tendered to Dr. S. York, for his able and interesting address; and that Drs. Davis, McClure and Hunt are hereby constituted a committee to solicit a copy of the same for publication.

On motion of Dr. S. W. Thompson,

Resolved—That the thanks of the Society be returned to Dr. C. Johnson for his able and impartial conduct as President of this Society for the past year.

On motion of Dr. Johnson,

Resolved—That the thanks of the members of this Society are cordially tendered to the trustees of the Baptist Church of this place for the use of their house during the various sessions of our Society.

On motion, the Secretary was requested to furnish a copy of the proceedings of this meeting to the editors of the Paris and Mar-

shall papers, Vincennes *Gazette*, and North-west Medical *Journal*, and that they be requested to publish the same.

The town of Marshall was selected as the most central point for holding the future meetings of the Society.

On motion, the Society adjourned to meet at Marshall on the 3d Wednesday in May next.

T. D. WASHBURN, M.D., *President.*

F. R. PAYNE, M.D., *Secretary.*

EDITORIAL.

Clinical Instruction.

In a recent number of the *Journal* we made some observations on the subject of Clinical instruction, calling in question the correctness of the often repeated assertion, that we have no *true clinical teaching* in the Hospitals of this country.

It seems that our remarks called out comments in the November number of the Peninsular *Journal of Medicine*, published at Ann Arbor, Michigan. Of the nature of these comments we have no knowledge, as that number of the *Journal* has never been received by us. But in the December number of the same journal, we find an editorial explaining more fully the writer's "views upon the subject of clinical instruction, and the manner in which it should be pursued."

That we may not misrepresent, we quote from the *editorial* in question as follows, viz.:

"In order to accomplish all these purposes, we can readily see what will be necessary.

"A visit to a patient with an acute disease, once or twice a week, will by no means suffice, however careful the examination, and full the opportunity of observing, at the time, may be. Neither will a daily visit accomplish the necessary purposes, unless the student has a full opportunity for a close personal inspection of the patient, and his various conditions, and has time to enquire into the effects of the medicine—and in short, every circumstance and condition of the case. The countenance must be observed—the tongue seen—the pulse felt—the condition of the skin noticed—the chest percussed—the breathing listened to—the pupils inspected—the excretions examined, &c.

"And further than this, the management of the sick room should be studied—the conduct of the nurse, the deportment of attendants

—the whole system of management of the mind, so important a part of correct treatment—the mode of using bathing, frictions, &c., and of preparing and administering food and medicines, must receive attention.

"It is also important, in true clinical instruction, that the student should see patients of a class, and under circumstances similar to those with which he will meet in practice. All must see that all these things are essential to correct clinical instruction of the highest kind.

"Now, we ask, are these circumstances possible—can these things be done in a hospital, in *any* hospital, on this or the other side of the Atlantic—in Paris, in London, in New York, in Philadelphia, in Cincinnati, or in Chicago? In the very nature of the case, the thing is impossible. In hospitals of large cities, we meet with a class of cases very different in their aspects and tendencies. from those which are met with in respectable private circles, and especially from those met with in the country; and students receiving their only impressions of diseases from such sources, will be greatly at fault in their management, when they meet with cases of a class so very different. and under circumstances so completely changed."

What a pity it is that our learned editorial *confrere* at Ann Arbor, had not enlightened the profession on this subject, a few years earlier. Had it been known earlier that the "circumstances" necessary for profitable clinical instructions, were altogether "impossible" in the Hospitals, not of America alone, but in those of "London and Paris" also, it might have saved many a poor fellow the necessity of spending his last dollar to cross the ocean and gain access to the wards of those Hospitals. Again it might have preventedthe assembled wisdom of our profession, as embodied in the American Medical Association, from committing so grave an error as to recommend and re-recommend from year to year, that the attendance on *Hospital* clinical instruction for at least one term, should be considered a *necessary* part of medical education, one o the requisites, indeed for admission to an examination for the degree. It is an old saying that "necessity is the mother of invention;" and who knows but the entire absence of a Hospital at Ann Arbor, constituted *the necessity* which determined the grand discovery announced in the last paragraph quoted above? But seriously, let us examine somewhat carefully the positions assumed so confidently by our worthy friend and co-laborer. And in doing

so, our readers will excuse us if we leave the Hospitals of Paris, London, Philadelphia, New York, &c., to take care of themselves, while we draw our illustrations directly from the one with which we are more immediately connected at Chicago. What then are the circumstances claimed to be necessary for true and proper clinical instruction? First, the student must have opportunity for a close personal inspection of the patient. "The countenance must be observed—the tongue seen—the pulse felt—the condition of the skin noticed—the chest percussed—the breathing listened to—the pupils inspected—the excretions examined, &c." This is right, perfectly so, and just what the students visiting the wards of Mercy Hospital in Chicago, *actually do* every morning. They are brought immediately around the bed of the patient; they not only observe the countenance, but their attention is called to each lineament and item which contributes to make up the sum of its expression; they not only see the tongue, but their attention is directed to each of its peculiarities; they feel the pulse and the skin; they auscultate the chest; and they inspect the excretions Take, for example, the actual clinical lesson for this morning. After noting briefly the progress of some cases fully examined at a previous visit, they come to the new cases for the morning. They first surround a man brought to the Hospital two days previous with typhoid fever of moderate severity. He is in the third week of the disease, having been but little interfered with by medicine of any kind. The history of his case is explicitly stated; all its diagnostic symptoms are fully examined; the changes, both general and local, which are supposed to have taken place are pointed out; then the indications for treatment, and the particular means for fulfilling them. They individually examine the countenance, skin, tongue, pulse, respiration, abdomen, and excretions. They feel of the rotund and tympanitic abdomen, and in this case see a few well marked *rose* spots.

Having finished this case they passed to another case. He too has typhoid fever, but farther advanced, tongue entirely dry, legs covered with purpuric spots, chronic abscesses forming in the cellular tissue. They examine this case with the same care as the other. Though the same form of fever, their attention is called

particularly to the difference between the two, and the corresponding variations in the therapeutic indications. From this they go to a third case of the same disease. It is a young man in the early part of the second week of the disease. The fever is well marked but mild and very favorable in its progress, but they find the *rose spots* thickly covering the whole abdomen and chest. The hour is past and the class are dismissed. Here they have had the most perfect access to three cases of typhoid fever, each illustrating a particular stage and grade of the disease. They have examined them personally, their attention being specially directed to the diagnostic symptoms and pathological conditions. They will note from time to time the progress of these cases until their final termination either in convalescence or death. But before these have gone from the wards, others of the same disease will come, and during the term they can examine and compare almost every form and stage of the disease. The same thing is equally true of the other forms of fever and dysentery, as well as the various local inflammations. It is only yesterday morning that we called the attention of the last division of the class to five cases of dysentery in the same ward and almost side by side.

One was a recent and active case, two had just passed into a chronic form succeeding acute attacks, and the other two had existed three or four months presenting the extreme emaciation, enemia, and edematous legs, which accompany the long-continued ulceration of the mucous membranes. It is but a few mornings since that we had grouped together at one time and in the same ward, three cases of opthalmia, one of slight conjunctivitis another of scrofulous inflammation of the tarsus of the eye-lids, and the third severe scrofulous cornietis with ulceration. As the peculiarities of each were pointed out the diseased eyes were subjected to the direst personal examination of each student. For another clinical group we had, not long since, cases of scabies, chronic exzema of the face and erysipelas of the face all under examination and treatment at the same time. We have now in the wards patients with pulmonary tubercular deposits in almost every stage from crude primary tubercles, to complete suppuration. These have been several times carefully auscultated personally by each member of the

hospital class, thereby accustoming their own ears to the variations of respitatory manner, vocal resonance, &c., which accompany this disease. The clinical lessons here alluded to are not selected ones, but simply such as have actually occurred within the last few days. They are sufficient however, to show, that the very circumstances and conditions set forth by the editor of the *Peninsular Journal* as necessary for true clinical instruction, are not only found in hospitals, but found there in a much greater degree of perfection than anywhere else in the world.

For where else is it posible to procure for the student that freedom of access to the patient for personal examination, and to such a number and variety of patients as shall illustrate not only different diseases, but also different forms and stages of the same disease? Our friend at Ann Arbor says that they are to be found "in every town, in every hamlet, in every country side' where there is an intelligent and well educated practitioner."

And again he says "*our position is, that private clinical teaching is most useful.*" Will Prof. Palmer, the author of the editorial in the Peninsular Journal, frankly tell us how many times, during his Professional life of 15 or 18 years, he has actually taken a Student to the bed-side of a respectable patient in private practice, and there detailed to him "the previous history of the disease and of the patient—the class of persons to which he belongshis individual peculiarities and tendencies—the circumstances with which the case is surrounded, &c.;" then make "a full *diagnosis* in the presence of the patient, pointing out and illustrating the means by which it is arrived at;"—also "the *special pathology* of the case—the indications of treatment, and the means by which those indications are to be fulfilled;" in the mean time allowing the student to *observe the countenance, see the tongue, feel the skin and the pulse, percuss the chest, listen to the breathing, and examine the excretions*, &c.?—And all this not once for the same patient, nor once in "*three or four days*," but *daily* from the beginning to the end of the case? Such an exercise would require the practioner to give a familiar lecture of, at least, half an hour directly over the patient. The simple statement of the question carries with it an obvious answer. Neither

Professor Palmer nor any of his colleagues have ever attempted such a task twice in their lives. They doubtless have occasionally taken a favorite pupil to see a patient with them, and allowed him to glance at the patient's tongue, feel the pulse, and look at the discharges; possibly also he has been allowed for one single time to put his ear upon the chest; and after retiring from the house a desultory conversation has been carried on in regard to the case,—we have done so, many times, and have doubtless thereby benefited to some extent our pupils. But this falls far short, in every respect, of what the Ann Arbor editor has himself described as constituting true Clinical Instruction; and equally far short of the Clinical instruction given in the Mercy Hospital every morning. It is very easy to talk about *Private* clinical instruction; and such *talk* may serve the special purpose for which it is designed, namely, to satisfy the medical students at Ann Arbor that they must look to their private preceptors, instead of the Faculty of their State University, for this most important department of medical instruction. How much of it, they will ever actually get in the several "hamlets and country rides," of Michigan, the experience of the last eighteen centuries and the common sense of every reader can easily estimate. But our friend Palmer betrays a great want of appreciation as to what *kind* of clinical instruction the student needs, when he talks about his watching "at the bed-side with *book in hand*," like a Homœopath looking for the correspondence between the symptoms of his patient and those of his supposed remedy. The very first clinical lesson he needs, is to be taught a systematic and thorough *method* of examining patients and diagnosing diseases.

He needs to be taught *how* to observe, and *what* to observe—to have pointed to him on the living patient the *import* and *value* of individual symptoms, their connections with and dependencies on each other; things which he cannot acquire except under the immediate direction of a prompt and thorough clinical teacher in the immediate presence of a variety of patients. This want of appreciation on the part of the Editor, is further shown by his quotation from Prof. Flint of Paris. To confound the *wants felt* by an experienced physician and teacher, who is abroad seeking for every thing relating to the minuter details of medical science and

philosophy, with the *wants* of the student preparing to enter on the active duties of his profession, is to betray at least want of reflection. His remarks about the absence of *proper time* for attention to clinical instruction during the college terms, would have some force were it not for the fact, that so long as the present system of education continues *it must be attended to then or not at all.* But the fault here is not with the clinical instruction or the hospitals, but with the arrangement of the college terms. Few are more conscious of the defects in the present general system of medical education than we; and none have labored more diligently to remedy those defects. It will in no wise aid the good work, however, to have our Michigan friends array themselves against one of the most important departments in the whole system. It would be far better to relieve themselves at once from their false position by transferring the medical department of their University to Detroit, where they could do justice to the students of their own state by having access to Hospitals. If this was done we should hear no more about the worthlessness of Hospital Clinical Instruction.

D.

The caption to the first article in the present number of the journal, should have stated that the paper of Dr. Thompson was read before the Esculapian Medical Society, and forwarded for publication by the Secretary in accordance with a vote of the Society.

The article we promised to give in this number, on Epidemic Jaundice, is deferred to make room for other communications.

Close of the Volume :—The present number closes the third volume of the New-Series of the North-Western Medical and Surgical Journal. The Journal has for several years been established on a firm basis, and has a wide circulation.

Owing to some temporary obstacles, the issue of the Journal has been more irregular during the past year, than we could have wished.

This source of annoyance however, will cease with the commencement of the new Volume. The present is a favorable time for subscribers who are in arrears, to pay up, and for new ones to forward their names and their money.

But hereafter all communications and letters relating to the Journal should be directed to Dr. N. S. Davis, Chicago, Ill.